sent in use in various countries are not always those ordinarily adopted. In Japan, for example, the present standard of mass is the "Kwan," prototypes of which were recently standardised at Sèvres.

We can, however, cordially recommend the book, which should prove very useful. J. A. H.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Thermodynamic Reasoning.

IN the address delivered by Principal Griffiths at York, which is printed in your issue of August 9, I read: "Prof. Armstrong remarks that it is unfair to 'cloak the inquiry by restricting it to thermodynamic reasoning, a favourite manœuvre with the mathematically minded.' He adds that such a course may satisfy the physicist but 'is repulsive to the chemist.' The inquiry, 'Why is the application of thermodynamic reasoning repulsive to the chemist?' naturally suggests itself."

This statement shows a strange misapprehension of my position. I have taken exception *to the restriction of the inquiry* to thermodynamic reasoning, not in any way to the mere application of thermodynamic reasoning. My objection was to formula worship. I still and shall ever object to it, for it is the bane of progress. As I said at York, physicists too nearly resemble the visitors to London who walk along the Strand and Shaftesbury Avenue and are content to look at the theatres from outside; they resemble those who admire the British Museum building but have no desire to examine the treasures within it.

If I did not misunderstand him, Mr. Whetham implied at York that it was enough for him that a certain thermodynamic expression was valid: what the condition termed osmotic pressure really is—whether a true pressure or whether, as I suggested, a negative pressure or thirst—mattered not a jot. A certain mathematical thermodynamic picture being painted, no other artist need apply. This does not seem to me to be the attitude a scientific inquirer should adopt. Whether I represent the opinion of chemists matters little: personally I am not willing to remain outside the Museum: I shall go inside, if possible, trusting that in some faint degree I may be able to appreciate the wonders within it.

At present, progress is not a little hampered by the fact that chemists and physicists cannot wander through the museums of nature looking eye to eye in complete sympathy with one another: surely we are destined to be the closest of friends; more should be done to cultivate an understanding; a confusion of tongues has arisen which keeps us apart: we must both strive to speak a simpler language. Together

"Let us inspect the lyre and weigh the stress
Of every chord and see what may be gain'd
By ear industrious and attention meet."

HENRY E. ARMSTRONG.

IT is the strength and weakness of thermodynamical reasoning that it connects different phenomena without the aid of theories about the mechanism by which the connection is effected.

In the discussion at York, Prof. Armstrong put forward certain arguments in favour of the view that solution is a chemical phenomenon, and osmotic pressure due to an attraction of the nature of chemical affinity. He used these arguments in an attempt to invalidate van 't Hoff's thermodynamic theory, which shows that, from the observed solubility phenomena of volatile substances, it follows that the ideal osmotic pressure of a number of particles of such substances in a dilute solution must be equivalent to the pressure which the same number of particles would exert as a gas occupying the same space.

In my reply to Prof. Armstrong I pointed out that the thermodynamic theory is quite independent of the particular view we may adopt as to the fundamental nature of solution, and the *modus operandi* of osmotic pressure. Osmotic pressure may, as van 't Hoff himself supposed, be due to the impacts of the dissolved molecules; it may, as Prof. Armstrong believes, be caused by chemical affinity; it may be produced by some other undiscovered cause. The thermodynamic reasoning avoids all such hypotheses, and connects directly the experimental facts of the solubility of gases with the osmotic pressure they would exert against a perfect semipermeable membrane in dilute solution.

I have never suggested that the ultimate nature of solution was a matter of no interest. It is the question of most supreme importance now outstanding in these subjects; but let us clear the issue before attacking it. We must recognise clearly that the relations indicated by thermodynamics and confirmed abundantly by experiment are among the established facts to be explained by a theory of the nature of solution.

It is for this recognition of the true position of the problem that I contend. The thermodynamic reasoning which connects the ideal osmotic pressure with experimental phenomena is not in question. That reasoning is confirmed by measurements of actual osmotic pressures and of freezing points. It can only be invalidated by a general attack on thermodynamic theory, such as that which was foreshadowed in Mr. Campbell's recent reconnaisance-in-force. I do not think any such attack has much chance of success. Osmotic phenomena seem to me to be entrenched in the strongest part of the vast lines occupied by the science of thermodynamics.

Cannot Prof. Armstrong agree to accept the thermodynamic reasoning as confirmed by experiment, and pass on to the further problem? Personally, I think that the evidence at present available is on the whole in favour of the chemical theory of solution and osmotic pressure—the theory which Prof. Armstrong supports; but there is work to be done before such a conclusion can be taken as established. May we not agree that it is better both for physicists and chemists to do such work than to waste their energies in attacking with inadequate artillery the well-fortified citadel of thermodynamics?

W. C. D. WHETHAM.

High Borrans, Westmorland, August 21.

The Iron Arc.

WHILE carrying on some experiments with the electric arc between iron electrodes, one of my students, Mr. H. D. Arnold, noticed that there was a certain critical P.D. at which an abrupt change took place in the conditions of the arc. Subsequent investigation has shown that the effect is closely analogous to the "hissing point" of the carbon arc. How close the analogy is may be seen from the following remarks. If the iron arc is started with a large external resistance and maintained at such a length that the current is well below one ampere, it burns with little or no sound, and its appearance in the neighbourhood of the anode is very diffuse and ill-defined. As the external resistance is gradually decreased, the P.D. falls and the current rises until a certain critical value, depending on the length of arc and size of electrodes, is reached. At this point a very small decrease in external resistance suffices to cause a sudden increase in current and drop in P.D., precisely as with the carbon arc. At the same time the arc contracts, a bright spot appears on the anode, and a characteristic hissing sound begins. Further increase of current is accompanied by a *continued decrease* in P.D. The hissing stage, in fact, begins at quite a different point on the P.D.-current diagram from that in the case of the carbon arc. If the experiment is carried out in the reverse order, starting with a large current, the discontinuity is encountered again, but not until the current has been diminished beyond the value that it had at the beginning of the hissing stage. Indeed, with arcs of 6 mm. and more, the current on the hissing stage can with care be decreased until it is smaller than its previous largest value on the quiet stage. Thus there are two possible values of P.D. for the same current and length of arc, one corresponding to the quiet, the other to the hissing stage.

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How closely the physical cause of this discontinuity resembles that in the case of a carbon arc is still in doubt, though investigations bearing on this question are under way. With the iron arc there seems to be no sharply defined crater, for each electrode terminates in a viscous, incandescent globule of what seems to be magnetic oxide of iron, from which the discharge takes place. Thus we have to do, properly speaking, not with an arc between iron electrodes, but with one between electrodes of Fe_3O_4. Even when the arc is hissing strongly, the discharge seems to take place from only a small area on the surface of the globule. Moreover, a large increase in diameter of electrodes is accompanied by only a small increase in the value of the critical current, which varies between 0·8 ampere and 1·5 ampere over a wide range of values of length of arc and thickness of electrodes. On the other hand, I have found no positive evidence that the discontinuity is not due to the presence of oxygen around the anode. A test with an exploring electrode showed that the effect is confined mainly, if not entirely, to the anode. Given an arc burning on the quiet stage in the neighbourhood of the hissing point, the hissing can be precipitated by shortening the arc, just as in the case of the carbon arc.

After the current has been increased somewhat beyond the hissing point, the arc begins to rotate rapidly, so that on the anode a ring instead of a spot of light appears. This is accompanied by a high-pitched squeak or whistle, which, as the current is still further increased, degenerates into a sputter, and this in turn into a steady, strong hiss, the ring meanwhile having disappeared. At the beginning of the "whistling stage" the arc has a curious tendency to jump back into the quiet stage, so that for an instant the hissing ceases, the current falls abruptly, and the P.D. rises several volts. If one begins to diminish the current immediately after one of these abrupt changes, the quiet stage can sometimes be maintained steadily, even though the current is far greater than that at which hissing normally occurs. It is not impossible that slight irregularities in the supply E.M.F. may in certain circumstances serve to precipitate the change from the one stage to the other, even though the current be not that at which the change normally takes place.

In conclusion, the question may be raised whether Lecher's observation of the discontinuous nature of the arc discharge between iron electrodes was not made on the hissing stage alone, and whether, as with the carbon arc, the discharge may not be perfectly continuous when the current is made sufficiently small. It is planned to repeat Lecher's experiment, making tests on both the quiet and the hissing stages of the iron arc.

Middletown, Conn., August 9. W. G. CADY.

Volcanoes and Radio-activity.

IN the *Popular Science Monthly* for June Major Dutton has an interesting article on the above subject, which was noticed in a recent issue of NATURE. Having been occupied lately with the study of volcanoes in connection with a more general inquiry into the cause of earthquakes, it occurs to me to point out that Major Dutton has overlooked the recognised distribution of volcanoes about the sea coast, which seems completely to invalidate his theory. If radium, which the researches of the Hon. R. J. Strutt have shown to be so abundant in typical rocks of the earth's crust, such as granite, were an exciting cause of volcanic activity, we should expect to find an abundance of active volcanoes in the interior of continents, such as the United States, Europe, Asia, Africa, Australia, and Brazil, which is contrary to observation.

T. J. J. SEE.

Naval Observatory, Mare Island, California, August 10.

The Radio-activity of the Chemical Elements.

IN connection with the emission, from the radio-active elements, of corpuscles with velocities below the critical velocity necessary for the ionisation of gases, it has occurred to me that such a form of radiation is possibly a fairly general property of the chemical elements. It is, I think, usually accepted that "γ" radiation always accompanies the projection of "β" particles, and the extreme penetration of the "γ" rays seems to be directly due to the very high velocity of the average "β" particle. As the efficiency of the "X" rays is due to the sudden negative acceleration of the unit electrical charges (*i.e.* the corpuscles) as they strike the anti-kathode, it appears quite possible that corpuscles, moving with comparatively low velocities, may yet be capable of causing a form of "γ" radiation of feeble penetrating power. The fact that the kathode stream, which can hardly penetrate the glass of the tube, is still able to set up very penetrating X radiation when given a sudden negative acceleration by impact with the platinum anti-kathode may perhaps be given as an instance in support of this idea. It seems probable that the photographic action of a beam of corpuscles (deviated away from the "γ" radiation by a magnetic field) may be chiefly due to a form of "γ" ray set up on contact with the plate itself. The several mysterious instances of the fogging of photographic plates left in certain conditions for considerable periods may be caused by a very feeble form of "γ" radiation set up by the impact of slow-moving corpuscles on the surrounding matter. Such evidence of these slow-moving corpuscles may be somewhat meagre and doubtful, but I think that, so far as the ordinary chemical elements are concerned, the emission of such corpuscles may be very much greater than the measured activities would lead us to suppose.

C. W. RAFFETY.

Streatham Common, August 25.

THE OXIDATION OF ATMOSPHERIC NITROGEN IN THE ELECTRIC ARC.

IN the year 1775 Priestley published his "Experiments and Observations on Various Kinds of Air," in which he showed that when a series of sparks was passed through air, the air became acid. The experiment was carried out by means of a glass tube, having one end closed with wax through which a wire was fixed, the open end being placed over a solution of blue litmus. Sparks were passed between the solution and the wire, and in a short time the blue litmus turned red. He further noticed the important fact that the water gradually rose up towards the wire. The observations of Priestley were shortly afterwards substantiated by Cavendish, and in 1893 Lord Rayleigh, with better apparatus and appliances, repeated the experiments which ultimately led him to the discovery of argon. Priestley attributed the acidity to the formation of carbon dioxide, but Cavendish, on repeating the work, proved it to be due to the formation of nitric and nitrous acids.

After the successful experimental work of Lord Rayleigh, attention was turned towards the production of nitric acid from atmospheric nitrogen. But it was undoubtedly due to Sir William Crookes, who as president of the British Association in 1898 directed attention to the gradual depletion of the world's store of nitrogenous products, that the importance of the fixation of atmospheric nitrogen was recognised by the scientific and commercial world. At the present time about 1·5 million tons of Chili saltpetre are annually exported, but those who have studied the question consider that at this rate of exportation the Chilian beds will be, at the latest, depleted by 1940. But as the population of the world increases, the quantity of nitrogenous material required for fertilising purposes advances in equal ratio. Sir William Crookes pointed out in 1898 that the world's growth of wheat was about 163,000,000 acres, which at the average of 12·7 bushels per acre gave 2,070,000,000 bushels. "But thirty years hence the demand will be 3,260,000,000 bushels. . . . By increasing the present yield per acre to twenty bushels, we should with our present acreage secure a crop of the requisite

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